

severally twisted in opposite directions. A straight thick wire passed through the ring, the weight of which afforded a ready means of varying the force necessary to balance the torsion of the wire. My first object was to prove that the force of the latter was, at any rate to some considerable extent, independent of the tension. Suppose that with this arrangement, the wire being horizontal, a balance has been effected when the ring has been turned about the wire as an axis three or four times. What will happen when the wire is further strained? I think it would be a natural expectation (apart from special knowledge) that the weight will rise; on the other hand, a knowledge of the law of torsion teaches (?) that there is no increase of the force sustaining the weight, which therefore will *not* rise. But who would suppose that, on the contrary, it would sink? Such, nevertheless, is what takes place. I continued increasing the strain, and the weight continued to sink. I had to go on lessening the weight again and again (by shifting the balancing cross-rod), in order to restore the horizontality of the ring; until at last there was scarcely any force of torsion left! To repeat the experiment of course the ring had to receive three or four fresh turns. I did so several times, always continuing, as I thought, to increase the strain. All the time the wire was absorbing the torsion, and did not break. I then thought to try the effect of a high initial torsion. But I did not seem to get any such by turning the ring more than five or six times. I then thought to see how much twisting the wire would bear. Expecting it every instant to break, I counted up to 100 half turns. *By this time the wire was quite slack!* I added another hundred half turns. The wire was now half an inch longer, without any strain having been kept on it except just enough to keep it straight. I went on twisting. At 218 one wire broke. The other then had only sixteen half-turns of twist in it, out of the 230 or more received. I afterwards went on twisting, mending each time that the wire broke, till the twist (quite visible under the microscope) amounted to sixteen turns per inch. The length kept on increasing. After breaking, the wire always untwisted one turn in four inches.

I feel myself here in presence of laws of which I know *nothing*; and my object in writing this short experience is to ascertain whether it is sufficiently in accord with what *is* known to cause no surprise to any one but myself. In that case I shall be greatly obliged to any one who will tell me where I can learn all about it.

J. HERSCHEL

Collingwood, October 4

I forgot to say that in no case did slackening of the strain reverse the sinking of the weight due to increase of strain.

The Magnetic Storm

By the mail just arrived from Australia I have received copies of the photographic traces produced by the declination magnetograph at the Melbourne Observatory during the magnetic storm of August 12 to 14, kindly forwarded by Mr. Ellery, the Government astronomer there.

A comparison of these curves with those from the Kew instrument for the same period shows that the disturbance commenced and ended at both places at the same time.

It is not easy however to trace much similarity in the two sets of curves, as the individual excursions of the magnet east and west of the normal position which form the record of the magnetic storm, cannot be at all times followed in both curves, but the periods of greater disturbance seem to have been simultaneous. For example, the commencement of the disturbance was well marked at August 11d. 8h. 10m. p.m. at Melbourne, which corresponds to 11d. 10h. 33m. a.m. G.M.T., whilst here (*vide* Mr. Ellis's letter in NATURE, vol. xxii. p. 361) it commenced at 10h. 30m. a.m.; then again the large deviation to the eastward noted in the Rev. S. J. Perry's letter in NATURE, which occurred here between 12d. 11h. 30m. a.m. and 12h. 30m. p.m., seems to have had its effect, as a movement of the needle at Melbourne to the westward between 12d. 9h. 15m. p.m. and 10h. 30m. p.m. The maximum deflection which exceeded the limits of registration of the instrument, I estimate to have taken place at 10 p.m. The corresponding G.M. times for the above are 12d. 11h. 38m. a.m., 12h. 53m. p.m., and 12h. 23m. p.m.; the maximum deflection recorded here seems to have been at 12h. 25m. p.m.

The disturbed period may be considered to have died out at Kew at 14d. 8h. a.m. G.M.T., and at Melbourne at about

14d. 7h. a.m., but there is no very distinctive movement which would enable us to fix this limit with accuracy.

These interesting comparisons are extremely satisfactory, for it is but recently that the Government of Victoria was considering the advisability of discontinuing the system of photographic registration of the magnetometers at Melbourne, and consulted the Kew Committee upon the subject.

A circular was accordingly issued to the leading physicists of Europe, and their replies being almost unanimously in favour of the continuance of the recording system, the Government erected a new magnetic observatory, and decided upon carrying on the work.

Mr. Ellery has also forwarded a month's curves for the purpose of assisting in the international comparison of magnetograms now being prosecuted by the Kew Committee.

The preliminary results of their investigations have been already indicated by Prof. Adams in his recent speech at Swansea (NATURE, vol. xxii. p. 416). G. M. WHIPPLE

Kew Observatory, October 2

Coral Reefs and Islands

I HAVE been greatly interested in Mr. John Murray's paper on coral reefs and islands published in NATURE, vol. xxii. p. 351. I hope you will allow me space to draw scientific attention to the fact that as early as 1857 I published a paper on the Formation of the Peninsula and Keys of Florida (*Am. Jour.* vol. xxiii. p. 46), in which I maintain that the theory of Darwin, although so beautifully (as I thought) explaining the phenomena of the Pacific reefs, *wholly fails to explain those of the Florida coast.*

In 1851 I spent the months of January and February on the Keys of Florida, assisting Prof. Louis Agassiz in his investigations on the growth of reefs and formation of keys in this region. An abstract of these investigations and their results was published in the Report of the United States Coast Survey for 1851, p. 145 *et seq.*¹

In this report Agassiz shows that the Keys and nearly the whole Peninsula of Florida have been formed by the growth of successive reefs, one beyond the other from north toward the south. In my paper above alluded to, and also in my "Elements of Geology," p. 152, I state further, that the reefs of Florida, if we accept Darwin's theory, are entirely peculiar. For according to Darwin barrier-reefs are formed *only by subsidence*, while on the Florida coast we have well-marked barriers with channels 10-40 metres wide where there cannot be any subsidence, for continuous increase of land is inconsistent with subsidence. Again, according to Darwin barriers and atolls always show a *loss of land*, only a small portion of which is recovered by coral and wave agency; while on the Florida coast, on the contrary, there has been a continuous growth of the Peninsula by coral accretion, until a very large area, viz., about 20,000 square miles, has been added.

I have attributed the formation of *successive* reefs from north toward the south to the successive formation of the depth-condition necessary for coral growth; and this latter, in the absence of any evidence of elevation, to the steady building up by sedimentary deposit, and extension southward, of a submarine bank within the deep curve of the Gulf Stream. The formation of barriers instead of fringes on a coast which has certainly not subsided—for continuous land-growth negatives the idea of subsidence—I attribute to the shallowness and muddiness of the bottom along this coast. Only at a distance of twenty to forty miles, where the depth of twenty fathoms is reached, and where, therefore, the bottom is no longer chafed by the waves, the conditions necessary for coral growth would be found, and here a line of reefs would be formed, limited on one side by the depth and on the other by the muddiness of the water.

In brief then, according to my view, the Peninsula and Keys of Florida were formed by the co-operation of several agents:—

1. The Gulf Stream building up and extending a submarine bank within its loop. 2. Corals building successive barriers on the bank as the latter was pushed farther and farther southward. 3. Waves beating the reefs into lines of islands. 4. *Débris* from the reefs and keys on the one side and the already formed mainland on the other filling up the successive channels and converting them first into swamps and finally into dry land.

Whether this view is true in all its parts or not, there can be

¹ This report has been recently published in full as one of the memoirs of the Harvard Museum of Comparative Anatomy, but I have not yet seen it.

no doubt that the southern coast of Florida affords exceptional advantages for the successful study of the formation of coral reefs.

JOSEPH LECONTE

Berkeley, California, September 18.

Geological Climates

THE dilemma into which Dr. Houghton thrusts the rigid uniformitarian school is one which was enlarged upon some years since, when reef-building corals were asserted, upon the evidence afforded by fossils, to have existed during the Miocene and Oligocene ages in seas where Tasmania now exists in the south and Hampshire in the north. There are no instances of large masses of reef-building corals in corresponding latitudes at the present day, and the range of these surface-living, high-temperature-requiring zoophytes is well known.

Uniformitarians may take comfort, however, and slip under the horns which Dr. Houghton so ably presents for their transfixment. Where I now write, on the Bagshot sands and gravels of Cooper's Hill, facing the cold north with a touch of the east, there is a patch of bamboo canes in full leaf. They were in full leaf at this time last year. The plant survived out of doors the extreme frost and fogs of last winter and other evidences of a temperate climate, and it has been in beautiful leaf all this summer.

Now everybody knows that in torrid India the bamboo grows. Therefore if the palæontologist of the year A.D. 18800 should dig up the Cooper's Hill stalks and leaves, and should have the opportunity of examining in some future Kew the bamboos of the hot parts of the earth, he would logically, geologically, palæontologically, but somehow unreasonably, come to the conclusion that Cooper's Hill and India enjoyed corresponding and intensely tropical climates in 1880, during the geological age when the earth's polar axis was certainly inclined nearly $23\frac{1}{2}^\circ$ to the plane of the ecliptic.

P. MARTIN DUNCAN

Royal Engineering College, Cooper's Hill, Staines, October 9

The Yang-tse, the Yellow River, and the Pei-ho

I HAVE been much interested in the paper on the above rivers, published in NATURE, vol. xxii. p. 486. To the extent of the writer's personal observations the calculations appear to have been careful and accurate, and as near the truth as the observations of a single year are likely to be. A reference to Sir Charles Hartley's observations of the Danube, extending over ten years, shows that the mean maximum discharge of that river for one year exceeded the minimum by 3 to 1.

It is however to the use of one observation of the Yellow River made in 1792 by Sir Geo. Staunton that I feel compelled to enter a protest, firstly, because one observation is misleading in drawing general inferences, and, secondly, is especially to be suspected when it is at variance with other well authenticated examples.

According to the writer of the paper, the mean discharge of the Yang-tse is 770,397 cubic feet per second, carrying to the sea 6,428,800,000 cubic feet of sediment per year, but the Yellow River having only a mean discharge of 116,000 cubic feet per second delivers, according to Sir George Staunton, 17,520,000,000 cubic feet of sediment per year into the Gulf of Pe-Chili. With Dominie Sampson we may well exclaim "prodigious!" It has struck me as an explanation of this anomaly that Sir George Staunton probably measured the deposit from "the gallon and three quarters" of the Yellow River water as *wet mud*.

If so this will at once account for the excessive amount of it. The deposit of Nile mud in the reservoirs of the Cairo water-works often amounts to 1 inch in 10 feet of water,¹ or $\frac{1}{10}$ part of the bulk. Dr. Letheby's analyses show that in August the proportion by weight of sediment (dried) being the maximum of the year, in Nile water is $\frac{1}{175}$ ²; thus taking the specific gravity of the dry mud at 1.9, the measurement of the wet deposit by bulk exceeds the dry about 10½ times.

If the 80 grains to the pint of the Yellow River water be divided by 10½, we arrive at between 7 and 8 grains per pint of dry sediment, corresponding closely with the proportion given by the writer for the Pei-ho and Yang-tse.

I would also point out that the discharge of the River Plate as given in the table is not the *mean*, which has not yet been

¹ "Mediterranean Deltas," *Edin. Review*, January, 1877.

² "Egyptian Irrigation," Second Report, January, 1876. By John Fowler, engineer to the Khedive.

ascertained, but the *dry weather flow*.¹ Still another little error, for which the writer is in no way responsible, being a quotation from Huxley's "Physiography." The discharge of sediment by the Thames is a calculation by Prof. Geikie on an *hypothesis*, not on observation; and instead of 1,865,000 should be 18,650,000—this printer's error has been copied from Geikie's original paper by writer after writer without discovery.

I should feel obliged if the writer would explain why the surface-current of the Yang-tse and Pei-ho should vary so in velocity with the same average depth of water. It seems anomalous.

T. MELLARD READE

Blundellsands, Liverpool

Miller's Elements of Chemistry—Part III. Organic Chemistry

IN his notice of the new edition of this work, by Mr. Groves and myself, which appears in NATURE, vol. xxii. p. 530, Mr. Muir refers to an obvious omission at p. 933. May I request those who possess the book to insert at the top of the page the words "Probably, however, the most weighty objection that can be raised to the" . . . Although in the revise, by some strange mischance this line has been dropped in printing off.

HENRY E. ARMSTRONG

Swiss Châlets

IDENTICAL suggestions to those of Mr. George Henslow with regard to the connection in descent of modern Swiss châlets with ancient pile lake-dwellings will be found expressed in Dr. J. J. Wild's "At Anchor" (Marcus Ward and Co.), p. 106, and with some detail in my "Notes by a Naturalist on the Challenger" (Macmillan and Co.), p. 399. Dr. Wild, who is a native of Switzerland, and I arrived at the same conclusions independently, as we only found out on reading one another's books, from the study of the modern pile dwellings of the Malay Archipelago during the voyage of the *Challenger*, and we both amongst other conclusions identified the balcony of the châlet with the ancient platform, as does Mr. Henslow.

H. N. MOSELEY

New University Club, St. James's Street, S.W.

Spectre of the Brocken at Home

HAVING occasion ten days ago to go into my garden about half past ten o'clock at night I found there was a thick white fog, through which, however, a star could be seen here and there. I had an ordinary bedroom candlestick in my hand with the candle lighted, in order to find the object I wanted. To my great surprise I found that the lighted candle projected a fantastic image of myself on the fog, the shadow being about twelve feet high, and of an oddly distorted character, just as the spectre of the Brocken is said to be. It is of course usual on going into the open air to use a lantern with a solid back for any light that may be wanted, and with this, of course, such a shadow would not be seen; but in this charmingly foggy valley of the Thames, and in these days of "Physics without Apparatus," the effect I saw can probably be seen only too often. May not the gigantic spirits of the Ossianic heroes, whose form is composed of mist, through which the stars can be seen, be derived from the fantastic images thrown upon the mountain fogs from the camp fires of the ancient Gaels? In a land where mists abound a superstitious people might very readily come to consider a mocking cloud-spectre to be supernatural, though it was really their own image magnified. If it be true that in our earlier stages of development we resemble more nearly the past forms of life and thought, I may mention in this connection that, thinking to amuse a little child of three, I threw a magnified shadow of her on the wall with a candle, and then, by moving it in the usual way, made the figure suddenly small. Instead of the changing shadow giving the pleasure intended, the child was terrified, as the warriors of Morven may have been when they saw their shadows on the clouds.

J. INNES ROGERS

Putney, October 8

Ice under Pressure

THERE is a point in Dr. Carnelley's letter (NATURE, vol. xxii. p. 435) which I have been hoping to see cleared up by subsequent letters. He says, "In order to convert a solid into a

¹ Report by James Bateman, C.E.